

TOWARD A THEORY OF VERBAL BEHAVIOR

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This paper is a reply to an accompanying set of six commentaries by Sidman; Hayes and Barnes; Schusterman, Kastak, and Reichmuth; Tonneau and Sokolowski; Lowenkron; and Moerk. Those commentaries were prompted by our article "On the Origins of Naming and Other Symbolic Behavior" (1996), which was, in turn, followed by 26 commentaries and a reply. In the course of the present reply, we further develop the naming account to embrace more complex verbal relations such as *same*, *different*, *more*, and *less*. We also examine what we see as the lack of conceptual coherence in equivalence theories, including relational frame theory, and the disparities between these accounts and the findings from empirical research.

Key words: naming, verbal behavior, language, equivalence, relational frame theory, mediation, match to sample

Our previous reply (Lowe & Horne, 1996) addressed issues raised in 26 initial commentaries on our (Horne & Lowe, 1996) account of early language learning and its relevance to recent research on stimulus classes, in particular those identified with the term *equivalence*. In this reply we tackle the main issues raised in six further commentaries, clarifying evident misunderstandings about our analysis and resharpening the focus upon what we see as the central concerns for future theoretical and experimental work. We begin by reviewing the objectives of the naming account and examine the criticism that it is *mediational*. We then consider some of the main points raised by Sidman, particularly concerning the relationship between equivalence theory and language and, more generally, the theoretical and empirical status of the concept of equivalence itself. In response to Hayes and Barnes, we attempt a more detailed evaluation of relational frame theory than we provided in the initial essay, and a direct comparison of the ways in which it and the naming account, respectively, explain derived

stimulus relations and the findings of relational frame research. We reexamine the evidence of Schusterman and colleagues as to the purported success of a sea lion in equivalence tests, and the arguments provided by Tonneau and Sokolowski in favor of molar theories of equivalence. Finally, we consider points raised by Lowenkron concerning the behavioral processes by which naming is established and more general theoretical issues aired by Moerk. We are grateful for the contributions of all the commentators, in both rounds of this discussion; without such sharing of ideas the advancement of theory and related empirical research would be slow indeed.

Equivalence, along with related issues, has featured prominently in the commentaries. This is hardly surprising, given that it has been the focus of considerable conceptual interest and innovative research within behavior analysis for some time. We are in no doubt that the legacy of much of this work will be positive. Murray Sidman, in particular, has made a central and lasting contribution towards ensuring that stimulus classification, or categorization, and the role it plays in language are put firmly at the top of the research agenda and, whatever the research procedures finally adopted in this endeavor, his methodological rigor and inventiveness will continue to serve as a model for us all. Thanks to Steven Hayes and colleagues, the targets of research have been broadened to include a range of derived relations. However, these advances notwithstanding, our appraisal of the concept of equivalence and its

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role in behavior analysis remains highly critical.

In any case, as we outlined in our target article, our prime concern is not with equivalence and its related issues, but rather with the development of an overall theory of behavior. Such a theory should embrace not only the *contingency-shaped* behavior of non-human animals but also complex human behavior, particularly the behavior that Skinner termed *rule governed* or that we term *verbally controlled* (Horne & Lowe, 1996, pp. 212–213). As Skinner recognized, an understanding of the distinction and interaction between these two basic types of behavioral relation is crucial in the analysis of human behavior. But such an analysis cannot proceed without an account of verbal behavior itself and of how it is incorporated into the broader behavioral system and so generates a range of phenomena that include verbal rules. This was the task Skinner set himself in *Verbal Behavior* (1957) and numerous subsequent writings and that we also set ourselves in our paper on naming.

For many years the need to make special accommodations to account for distinctly human functioning went largely unrecognized within behavior analysis. Indeed, it was widely assumed that the functional determinants of operant behavior in humans were no different from those that applied in other animal species (see Lowe, 1979, 1983). Human subjects routinely participated in experimental investigations of the effects of schedules of reinforcement, and researchers discussed the results in terms of the latter contingencies, just as they did similar procedures with animals. Unfortunately, however, there were substantial differences between the animal and human sets of data that unless they were explained seemed to rule out straightforward contingency analyses. Progress began to be made only when investigators acknowledged that the verbal repertoire that verbally able human subjects bring with them to an experiment inevitably transforms the experimental environment into one that is also substantially verbal. Whether or not these subjects are explicitly provided with experimental instructions, they instruct themselves about their own behavior and its outcomes. For them, many features of that environment, including the experimental procedure itself, occasion

naming and complex forms of self-instruction. Such verbalizations give rise to a range of behavior patterns that are never seen in the animal literature (Bentall, Lowe, & Beatty, 1985; Catania, Horne, & Lowe, 1989; Catania, Matthews, & Shimoff, 1982; Horne & Lowe, 1993; Lowe, 1979; Lowe, Beatty, & Bentall, 1983; Lowe & Horne, 1985). Quite simply, much of human operant behavior, not least that studied under laboratory conditions, is rule governed or verbally controlled rather than contingency shaped (Lowe, 1979). This “language hypothesis” is now widely accepted (see Hayes & Hayes, 1992), and there seem to us no good grounds for assuming that human behavior on conditional discrimination tasks should be any different. It is implausible that human subjects in these conditions would respond only in accordance with the experimenter-defined contingencies of reinforcement and not also to their own verbal behavior about these contingencies (see also Remington, 1996; K. Saunders & Spradlin, 1996). Indeed, the need to consider the effects of verbal control is particularly compelling in this domain given that (a) the differences between humans and other animal species are even more clear-cut in match-to-sample than in schedule performance; (b) the involvement of verbal behavior in the forming of arbitrary stimulus classes in match-to-sample procedures is so well documented (Horne & Lowe, 1996, pp. 215–227); (c) the operation of verbal effects on match-to-sample performance would be consistent with what we know about the rest of human operant performance. We suggest, therefore, that until it addresses the role of verbal control, conditional discrimination research with humans will make little significant progress.

In our target article we addressed language development up to the point when children begin to combine names, usually during the 2nd year of life. We hoped, in the course of describing what takes place during this phase of development, to specify *naming*—in our view, the higher order behavioral relation that is the basic behavioral unit of language. As a central feature of that specification, we aimed to show how naming classifies or categorizes objects and events and is the basis for rule-governed or verbally controlled behavior, but we did not attempt to go beyond

this to deal with autoclitic verbal behavior (Skinner, 1957). However, in order to respond to some of the commentaries in the present round (see Hayes & Barnes), we have developed the account to embrace some of these more complex verbal relations.

NAMING AND MEDIATION

Because it involves naming, ours is thought by some to be a *mediational* account of equivalence (see commentaries by Sidman and by Tonneau & Sokolowski; see also Hayes, 1994; Hayes & Hayes, 1992; Stromer & Mackay, 1996; Stromer, Mackay, & Remington, 1996). Perhaps because many authors identify *mediational* with the kind of mental constructs criticized by Skinner (1950), the term is not often used without pejorative overtones. But there is an important distinction to be made: Unlike an unobservable hypothetical construct, verbal behavior is *behavior* that can occur equally well in overt or covert form. To include it in an explanatory system is thus not to invoke some shadowy construct that exists at another level of explanation in which the subject matter is measured in different dimensions (Skinner, 1950). And clearly, it would be an odd account of human behavior that excluded explanation of language and its effects on other behavior. Indeed, were prejudices against this type of mediational account to hold sway, then much of *Verbal Behavior* (1957) and Skinner's other writings in this area would have to be discarded. Out, for example, would go all of his work on the autoclitic, rule-governed behavior, and verbal thinking. For Skinner, whether one appeals to mediating events, overt or covert, is not an ideological matter but depends upon whether the functional analysis demands it. *Verbal Behavior* is replete with examples for which it is necessary to infer such mediating behavior; not to make such inferences, he argues, would leave embarrassing gaps in the account. For example, he writes, "When someone solves a problem in 'mental arithmetic,' the initial statement of the problem and the final overt answer can often be related only by inferring covert events. We have to account for verbal behavior which is under the control of covert speech—which reports it or qualifies it with autoclitics" (1957, p. 434).

More specifically, Tonneau and Sokolowski

suggest in their commentary that our position here is comparable to Pavlovian mediational theories that are based on unobservable inferred stimulus-response chaining. This indicates a misunderstanding not only of our account of verbal behavior but also of Skinner's. The naming relations we describe, incorporating as they do stimulus classes and bidirectionality within an operant account, are far removed from anything described in such mediational theories. Tonneau and Sokolowski, however, propose that in the naming framework each covert verbal response ("a") can be traced back to some prior overt behavior ("A"), much as in Pavlovian mediational accounts of the behavior of nonverbal animals (e.g., Holland, 1981, 1990), in which environmental events (A, B, and C) are supposed to evoke covert representations (a, b, and c) in the subjects. If this be the case, they argue, why not establish what were the relevant overt behaviors and confine the account to the overt domain? But the naming and other verbal behavior that feature in our account occur both overtly and covertly; indeed, in young children they exist initially at the overt level alone and only later occur covertly as well (Horne & Lowe, 1996). And like Skinner (1957) and Ryle (1949a), we maintain that verbal behavior, regardless of whether it is overt or covert, is not a ghostly echo of environmental events. It is free-operant behavior. The verbal responses it incorporates bear no formal resemblance to, and occur in a different modality from, the environmental stimuli that evoke them. In addition, verbal responses to stimuli can occur many times and in any order and are entirely free of the temporal ordering that is critical to standard mediational accounts. Verbal behavior is, in short, certainly not an unobservable hypothetical construct such as features in the mediational theories of Holland and others. And as far as its effects on an individual's other behaviors are concerned, it is largely irrelevant whether it occurs covertly or overtly.

This is not to say, however, that covert chaining of sorts is never involved in match-to-sample experiments but that, if it occurs, it is likely to be *intraverbal* "chaining" (Skinner, 1957, p. 423; and see Horne & Lowe, 1996). In addition, we maintain that it is precisely because intraverbal behavior and complex autoclitic behavior are sometimes in-

volved, and such behavior takes time to emit, that nodal distance and other similar effects occur in studies of stimulus equivalence and relational frames (Horne & Lowe, 1996; Lowe & Horne, 1996; and see Relational Frame Theory and Naming, below). On the other hand, Horne and Lowe (1996) provide numerous examples for which intraverbal behavior is not necessary but common naming may suffice. A single common name can be a very quick and highly effective means of classifying physically different stimuli in conditional discrimination tasks. We stress, however, that the main focus of interest for us is not how subjects succeed on match-to-sample tasks, but the classifying and categorizing behavior inherent in naming itself. There is no mediation involved here. Indeed, we have argued that every time individuals name an object they directly classify it (Horne & Lowe, 1996; and see Skinner, 1957, pp. 107–129).

There is a real sense, on the other hand, in which Sidman's concept of equivalence and the various frames and abstract concepts of relational frame theory can be said to mediate language, symbolic functioning, or verbal behavior. In our view the conceptual terms that Sidman and Hayes employ do not refer to behavior but are redescriptions of behavioral relations then used to explain the behavior from which they were derived. And because, in both their accounts, equivalence is regarded as a prerequisite for language, it is implied that we cannot look directly at the functional relations between the environment and different forms of verbal behavior but must do so instead through the mediating concept of equivalence. Explanations in relational frame theory rely on elaborate framing mediations that intervene between the environment and behavior. To take just one of many possible examples, Hayes, Gifford, and Wilson (1996) write,

For example, imagine a situation in which a group of boxes are waiting on the lawn to be placed into a moving van. On what basis are properties of the boxes selected, in order to determine the order in which to move them? According to weight, to size, to fragility of contents? The dimension(s) controlling behavior may be verbally described—may acquire functions by participating in relational frames—and may then in turn constitute the basis for additional responding. (p. 294)

What does the mediation of “acquire functions by participating in relational frames” between the objects and behavior possibly add to our understanding of the relations involved here? A virtue of the naming account is that it dispenses entirely with such mediation.

CAN EQUIVALENCE THEORIES ACCOUNT FOR LANGUAGE?

Mouse is a syllable. Now a mouse eats cheese; therefore, a syllable eats cheese. Suppose now that I cannot solve this problem. . . . Without doubt I must beware, or some day I shall be catching syllables in a mousetrap, or, if I grow careless, a book may devour my cheese! (Seneca, *Epistulae Morales*)

In his contribution, Sidman is sympathetic to Dugdale and Lowe's (1990) suggestion that naming involves a symmetric relation between the name and the thing named. Indeed Sidman goes further and proposes “the relation between names and the stimuli that occasion them to be not only symmetric but reflexive and transitive as well” (p. 262). Thus Sidman's view, shared by Hayes, is that a name is equivalent to the stimulus that occasions it, and the latter is the meaning of the name (Sidman, 1994, pp. 343, 346, 365; and see Wulfert & Hayes, 1988). Much of our target article, however, was based upon a recognition that an account of naming in terms of stimulus–response symmetry or equivalence simply did not fit the facts. According to Sidman's account, behavioral symmetry exists when, for example, subjects trained in a conditional discrimination procedure to emit a selection response to Stimulus B upon presentation of Stimulus A, proceed without further training to emit the same response to A when presented with B; that is, any pair of symmetrically related stimuli are functionally substitutable or interchangeable, and evoke the same response form. In his commentary he cites as an example of object–name symmetry, a child naming a boy. But symmetry in this context would yield the following: The child, having learned upon hearing */where's the boy?/* to look at and point to a boy, should then upon seeing a boy look at and point to the auditory stimulus */boy/*; it is clear that within the symmetry relation there is no be-

havioral basis for the child to emit the *vocal* response "boy." And similarly with the reverse condition: Having learned to emit the vocal response "boy" when she sees a boy, there is no behavioral basis for the child's emitting a response of an entirely different form (i.e., looking at or pointing to a boy, when she hears /boy/). The problem encountered by such a symmetry account is that naming has a variety of behavioral components; for example, objects and events evoke speaker behavior (e.g., vocalizing or signing), which produces stimulation that evokes listener behavior such as orienting, pointing, reaching, and so forth. Accordingly, the boy and the auditory stimulus /boy/ cannot be functionally substitutable because each evokes different behavior from the other; the boy evokes a vocal response, whereas the auditory stimulus /boy/ evokes behavior of a very different form such as orienting and pointing.

That there is a complete absence of any form of symmetry between the speaker and listener components of the name relation becomes even clearer when one considers that when a child sees a stimulus (e.g., a boy), this may evoke a particular vocal response (e.g., "boy"), but the stimulation (/boy/) thereby produced can evoke a range of disparate responses relating not just to that particular stimulus (i.e., the boy just encountered) but to the entire class of events (e.g., the accumulated characteristics of boys in general) encompassed by the name relation concerned (and see Skinner, 1957, p. 117).

This absence of symmetry is, in fact, at the heart of the fundamental differences that exist between names and the objects named. Naming is, after all, categorizing behavior relating to classes of objects and events. But an object such as, for example, a tree, does not classify or categorize. In addition, because a name evokes orienting behavior not only to the particular stimulus that occasions it but to an indefinite class of objects or events, a particular referent can *never* be the meaning of a name (and see Hunter, 1974; Ryle, 1949b; Scruton, 1994; Skinner, 1957, pp. 110, 117). Whenever naming is evoked, there is a behavioral transition from the particular to the general; naming is inherently generalizing, categorizing behavior. Herein, we suggest, lies the solution to the problem that Sid-

man ponders in his commentary when he asks, "how does language help us to abstract, to generalize, to analyze, and to synthesize, and how does it come to do so?" (p. 263; and see Horne & Lowe, 1996).

The fact that naming relations cannot be described in terms of symmetry or equivalence casts considerable doubt on Sidman's important claim that the formation of an equivalence class permits us to say that "semantic correspondences" have been established and that each of the class members "have the same meaning or that each is the meaning of the other" (Sidman, 1994, p. 343; see also Hayes & Hayes, 1992; Wulfert & Hayes, 1988). A hypothetical example serves to illustrate the extent of the difficulty. Suppose that English-speaking subjects are presented with novel graphic stimuli. On some occasions when shown Stimulus A, their selection of Stimulus B from a range of alternatives is reinforced; on other occasions when shown Stimulus A, their selection of Stimulus C is reinforced. A large number of such conditional discriminations are established. The novel stimuli are in fact Chinese characters; those stimuli presented as samples are questions, and the reinforced selections are the corresponding correct answers. Thus, presented with Stimulus A, "which is furniture?" (in Chinese), the subjects respond correctly when they select Stimulus B, "chair," or Stimulus C, "bed" (both also in Chinese). What then would they have learned? Although they may indeed respond correctly to the "questions" with "answers" as well as might be expected of native Chinese speakers, as Searle (1980) has argued, there are no grounds to suppose that they have in fact learned the semantic relations represented by these questions and answers, or any Chinese. Thus far, this conclusion appears to be consonant with the equivalence theories of Sidman and Hayes: Because all of the relations have been directly trained, there can be no evidence of symmetry or transitivity and, hence, the "linguistic prerequisite" (Sidman, 1994) is missing. But assume the subjects go on to pass the necessary tests of symmetry and transitivity. When presented with "bed" they respond either with "which is furniture?" or with "chair." According to equivalence theory the subjects' success here should provide the evidence that semantic relations

have been established. In fact, of course, now having "succeeded" on the equivalence tests, the subjects' selections are even less consistent with the semantics of Chinese than they were during initial training. This example clearly demonstrates that the existence of symmetrical or equivalence relations between stimuli cannot tell us which stimulus is the name of which, or indeed whether any particular linguistic relations are involved. This would be equally true if the subjects of such an experiment were sea lions or computers.

Further evidence of the impossibility of accounting for language in terms of either equivalence or relational frames attends claims that the use of words such as "is" or "is called" establish symmetry and a frame of coordination (i.e., equivalence) between words and objects (Hayes et al., 1996; Hayes & Hayes, 1989, p. 169). Thus, Hayes et al. (1996) write, "For example, suppose a child is told 'this is called a ball.' If this is called a ball, then a ball is this. That is, the words 'is called' quite reliably predict reinforcement for symmetry in this context" (p. 288). But whoever says "a ball is this"? And if there really were symmetry, what should "emerge" would be the observation "a ball is called this" which, of course, would not make sense at all. In short, if the word "is" really were able to establish equivalence or a frame of coordination between terms so related, then Seneca's quandary, outlined in the opening quotation of this section, would be real.

Conclusion. We believe that the view that there is a symmetrical relation between names and objects has arisen at least in part from the absence of a clear specification of what a *name* is. For example, the word is variously used as a term for the vocal response to a stimulus, the auditory stimulus produced by the vocal response, or a combination of both. When the behavioral components of naming are clearly specified, however, it is not difficult to see that there can never be symmetry between names and objects (see also Catania, in press).

THE CONCEPT OF EQUIVALENCE

Much of Sidman's commentary is devoted to a discussion of definitional aspects of equivalence, but just as we have questioned

whether the concepts of symmetry and equivalence are of value in the analysis of linguistic relations, it is similarly pertinent to ask whether these concepts are of value in accounting for performance on the conditional discrimination tasks from which they were originally derived. We also question whether *equivalence* is any longer a coherent concept.

Definitional problems. R. Saunders and Green's (1992) paper on the "Nonequivalence of Behavioral and Mathematical Equivalence" dealt a considerable blow to the notion that one could simply translate mathematical into behavioral equivalence. They showed, for example, that generalized identity matching cannot be used to test for reflexivity (cf. Steele & Hayes, 1991). The implication of this is that among all the many studies purporting to show stimulus equivalence there are none, in fact, in which all three criteria for equivalence enjoined by Sidman are satisfied. According to the operational definition, we have therefore never had an established instance of stimulus equivalence! Saunders and Green showed, in addition, that even where the tests prove to be "negative," subjects may nevertheless have classified the stimuli in ways that meet the equivalence criteria of symmetry and transitivity. Their paper concluded that tests for equivalence are thus neither definitive nor exhaustive and that "stimulus equivalence specifically, and stimulus-stimulus relations in general, are far more complex behavioral phenomena than the invocation of the mathematical analogy implies" (p. 238).

What is even more problematic for equivalence theories is that recent research has reported behavioral relations that should not exist were behavior to follow the orderly "regularities" of the mathematical relations to which Sidman alludes in his commentary. Studies by Pilgrim and Galizio (1995) and by Pilgrim, Chambers, and Galizio (1995), for example, have shown a dissociation between baseline and symmetry versus transitivity-equivalence patterns of responding on a match-to-sample procedure. According to equivalence theory, the relation between stimulus pairs, including which particular pairs are symmetrically related, should be specified by the test for equivalence, but in these studies equivalence and symmetry go in different directions and relate to different

stimulus pairings. Pilgrim and Galizio (1995) conclude that these findings cast into doubt "the nature of equivalence as an underlying, fundamental, and integrated process" (p. 237). Other studies (for reviews, see Horne & Lowe, 1996; Pilgrim & Galizio, 1995, 1996; R. Saunders & Green, 1992) have shown that (a) extensive training on conditional discriminations may be entirely unsuccessful in establishing success on any of the tests; (b) success on some or all of the tests may depend upon the particular subjects being studied; and (c) some or all of the tests can be passed in the absence of any conditional discrimination training.

In his commentary Sidman indicates how he has radically revised his original theory in order to account for some of these findings but, as we have argued (Horne & Lowe, 1996), most of the serious problems remain, not least the core problem of how to provide a coherent behavioral definition or specification of equivalence. Indeed, we have proposed that in trying to put right the deficiencies in the theory, additional major problems have been introduced. This is a view that is now also shared by Hayes and Barnes who, in their commentary, argue that the concept of equivalence has broken down, leaving "only the concept of partition or class standing . . . and that was a concept we had before equivalence arrived" (p. 238). How the abandonment of the concept of equivalence, upon which relational frame theory was largely based, affects that latter theory is a matter to which we shall return.

Problems with the data. Sidman proposes that his account of equivalence relations provides a useful framework within which to organize the existing data and principles of behavior analysis. Here, however, we briefly summarize what we see as the main empirical deficiencies of his theory, most of which are common to relational frame theory. It does not account for (a) major differences between humans and nonhumans in their success on tests of equivalence, or (b) the finding that human subjects' success on equivalence tests is related to their linguistic skills. Contrary to what is suggested by Sidman in his commentary, we maintain that there is no good evidence that subjects without naming skills have ever passed equivalence tests. It has been acknowledged that to furnish as evi-

dence postexperimental naming tests that, although they purportedly show absence of common naming, at the same time ignore intraverbal naming and autoclitic behavior, is not convincing (K. Saunders & Spradlin, 1996, p. 30; see also de Rose, 1996; Galizio, 1996; Pilgrim, 1996; Remington, 1996). The theory also does not account for (c) rule-governed or verbally controlled success on equivalence tasks and a possible distinction between this and contingency-shaped performance (and see Relational Frame Theory and Naming, below); (d) why it is often necessary to provide prolonged testing of equivalence before subjects show "correct" performances (Pilgrim et al., 1995; Pilgrim & Galizio, 1995; and see Relational Frame Theory and Naming, below); (e) why performances on equivalence tests are an inverse function of nodal distance (Fields, 1996) or relational frame complexity (Steele & Hayes, 1991; and see Relational Frame Theory and Naming, below); and (f) how *context* determines success on equivalence tests. Neither do equivalence theories contribute much to prediction and control. According to Sidman, the fact that stimulus classes are ever observed in the laboratory is itself a mystery unless one takes into account context. He maintains that if we do get the experimenter-defined classes it is because the context, and perhaps also the response and the reinforcer, "drops out" of the equivalence relations. And if we do not, and instead all the stimuli in the experiment form one large overarching class, then this will be because the context has not dropped out. The question of how this dropping out occurs and the conditions under which it might or might not happen, Sidman does not address. Indeed, he goes so far as to assert, "The mathematics does not pretend to provide a basis for predicting whether or not a particular set of baseline conditions will generate equivalence relations. It only tells us how to find out whether particular event pairs belong to that relation" (1994, p. 540). Context, both past and present, is certainly responsible for subjects' behavior in equivalence experiments, relational frame studies, and indeed just about all experiments in psychology. The problem is that if context is everything it is also nothing when it comes to prediction and control; its role should be specified in ways that serve a functional anal-

ysis. Finally, (g) we have argued that equivalence theories have failed to account for language itself, or advance in any way Skinner's (1957) exposition.

These are seven key issues that any theory in this domain must address, and in our naming account (Horne & Lowe, 1996; Lowe & Horne, 1996) we have attempted to do so. They expose what we believe to be another fatal flaw in equivalence theories, namely, that they fail to account for the data.

RELATIONAL FRAME THEORY AND NAMING

In the previous section we listed seven phenomena that we believe pose major difficulties for equivalence-based theories, including relational frame theory. We have asked two further questions of the latter: What is the history that gives rise to a frame of coordination? And how does the history work? In our view, neither of Hayes' commentaries seriously address these issues. Hayes has, however, raised questions for the naming account to answer. What we aim to do here is to respond to the points raised in the Hayes and Barnes commentary and, in so doing, to take the opportunity to compare naming and relational frame theory explanations of how classifications such as *same* and *different* and *more* and *less* are acquired. We also compare naming with framing accounts of relational frame theory research.

Stimulus Classes and Stimulus Relations

We first need to clarify a conceptual point raised by Hayes and Barnes. They draw a distinction between stimulus classes and stimulus relations and criticize Sidman and ourselves for focusing upon the former at the expense of the latter. Their disenchantment with an approach that is primarily class based seems motivated by a desire to distance themselves from the concept of equivalence, which they consider to have collapsed. We find this ironic, given the centrality of that same concept to relational frame theory as it has been described in the past. For example, Hayes (1991) has written of equivalence that it is "the most common and fundamental type of verbal construction" (p. 32) and, according to Barnes (1994, pp. 102–103),

Hayes views equivalence as the most important relational frame because it is central to the occurrence of referential relations in natural language . . . and is therefore necessary before derived relations can be learned. It would be quite difficult, for example, to teach a child that "hot" is the opposite of "cold" if the words "hot" and "cold" did not participate in equivalence relations with the physical events of hot and cold respectively (see Hayes & Hayes, 1989, pp. 174–175).

How then, if the crumbling concept of equivalence is the very foundation on which the relational framing edifice has been erected, can the latter theory itself survive?

We think that this attempt to disavow the concept of stimulus classes is mistaken because, like Sidman, we consider that understanding how organisms come to form categories is central to understanding complex human behavior in general and language in particular. To neglect the study of stimulus classes would be to remove the foundations for any coherent theory of human behavior. In any case, stimulus classes do not form of themselves; on the contrary, in studying them we are studying behavioral relations. And besides, as Barnes (1994) has noted (see above), without class-based concepts one cannot study derived relations.

How Do We Come to Classify Objects or Events as the Same or Different?

The naming account differs radically from relational frame theory in its approach to this question. But before we look in detail at these differences we should outline how each account approaches the question of how a verbal response comes to be related to objects and events. In the naming relation, as we have described it, each exemplar of a class of objects or events evokes a common verbal response (i.e., speaker behavior) that in turn evokes listener behavior (e.g., orienting to exemplars of the stimulus class). This bidirectional sequence of behavior can be initiated in a variety of ways, including, for example, by the subject's seeing an object or event, or by her hearing the common verbal stimulus, spoken either by herself or by another. Once such behavior (a naming relation) is established as a free-operant class, it occurs in varied contexts, its functions evolving through interactions with the environment. Through

naming, verbally able humans categorize features of their environment, thereby profoundly altering the ways in which they respond to it (Horne & Lowe, 1996).

Contrast this with the problems faced by a child who has to learn a *frame of coordination*, as posited by relational frame theory. In order to learn an arbitrarily applicable relational frame of coordination the child must be presented with an object (e.g., a dog) and a verbal stimulus (e.g., /dog/) in the presence of a contextual cue (e.g., "is called") that specifies that these two stimuli should be interrelated or treated as if they were the same as each other. As we understand it, central to the relational frame account is the notion that it is the contextual stimulus *exclusively* that exerts control over the *relational response*; the latter is not occasioned by the formal properties (or physical characteristics) of the stimuli to be related (see Hayes & Wilson, 1996). Thus, in the above example of the dog, "saming" (or coordination) would be under the exclusive control of the contextual stimulus "is" or "is called." But, in that case, how would the child name objects in her environment if there were no one there to provide the contextual cue that controls this behavior (e.g., "that is called a dog")? And in the absence of such a cue how would she, upon seeing a dog, ever say "dog," and if she does not, where is the verbal stimulus to form a relation of coordination with the dog itself? Even if she were somehow to utter "dog," relational frame theory would have it that in the absence of the contextual cue the relation of coordination would nonetheless fail to occur. That is, although a child could learn to emit a vocalization upon seeing an object, the two events could not be further related without a contextual cue being present to determine which of the many possible frames (e.g., "saming," "moring," "lessing," "oppositing") might apply. In brief, without such contextual cues she would be unable to name anything in her environment or, by definition, emit any verbal behavior, but instead would respond at the level of a nonverbal organism. This seems to us to be a serious flaw that runs right through the relational frame account; relational framing is dependent upon contextual cues that have to be provided in order for the relating to occur.

There is another possibility: If the verbal

community had taught the child to say "dog" upon seeing a dog and to orient to dogs upon hearing the verbal stimulus /dog/, then the dog in and of itself would be sufficient to occasion the child's naming of it. The child would thus be free, within the behavioral bounds set by the verbal community, to name, and this naming would influence other aspects of her reacting to any stimulus in her environment. But this is, of course, not as relational frame theory would have it but rather is Horne and Lowe's naming account of the matter. The distinction between naming and framing is nicely captured in a passage by Mead:

The dog only stands on its hind legs and walks when we use a particular word, but the dog cannot give to himself that stimulus which somebody else gives him. He can respond to it but he cannot himself take a hand so to speak, in conditioning his own reflexes. . . . Now it is characteristic of significant speech that just this process of self-conditioning is going on all the time. (1934, p. 108)

We would argue that to be bound by relational frames would leave one in no better position with respect to language than the dog in Mead's example.

Naming, categorizing, and contextual control. A key problem for all accounts in this area is how categorizations or, to use Sidman's term, *partitions*, of arbitrary stimuli are established. For relational frame theory there is the particular problem of how to account for contextual control of such behavior. Within the naming account, names are key determinants of contextual control. For example, if grapes, apples, bananas, and oranges are the items encompassed by a child's name relation "fruit," when she hears /pass me the fruit/ this evokes her selecting, from an array of objects that includes many nonfruit items, only those objects that feature in that name relation (i.e., grapes, apples, bananas, and oranges). Names thus directly specify object classifying (see Horne & Lowe, 1996, pp. 199–208). But it is not clear how relational frame theory could account for such contextual control. In order for these items to be selected, the auditory stimulus /fruit/ would have to be placed in a frame of coordination with each of the fruit items. But which component of the instruction /pass me the fruit/ would serve to establish the frame? Given that the contex-

tual cue for coordination must be *extrinsic* to the items to be coordinated, it could not be the auditory stimulus /fruit/ itself. This leaves only /pass/, /me/, and /the/, none of which could serve this function. We maintain that in such cases, yet again, the contextual cue that is vital to enable framing to occur cannot be specified by the theory.

But Children learn not only to name objects and events but also their properties. As Skinner (1957) described, the naming of a property such as redness is established by members of the verbal community teaching a child to say "red" in the presence of a variety of red objects for which the only reliable accompaniment is that property, and, similarly, to respond appropriately to /which is red?/ questions. Having learned to name a number of stimuli within the red wavelength range as "red," the child is able to extend this categorizing name to all red objects, irrespective of their other characteristics. In like fashion she learns to name other colors, shapes, sizes, numbers, and so on. Such a repertoire of property names then provides her with an enormous variety of contextual possibilities; the same set of objects may be categorized (or partitioned) in many different ways, depending on the name she (or someone else) applies to them: "red," "sharp," "smooth," "big," "noisy." In this sense, any object may participate in not one but many name relations and thus may be contextually related to others in a variety of ways through the operation of any one, or some combination, of those name relations. When a new function is acquired for any member, it may transfer to other members of the class in accordance with the particular name relation concerned (see Horne & Lowe, 1996, pp. 204–207). This straightforward account of how children learn to categorize objects in terms of their properties contrasts with that of relational frame theory, which, we have argued, has considerable difficulty in explaining such contextual control.

Naming "same" and "different." At around the time when they have acquired about 50 name relations, children are spontaneously able to sort on the basis of identity; they show this before they begin to show intraverbal naming or learn to produce the name "same" (Gopnik & Meltzoff, 1992). With respect to oddity, several studies of infants' ori-

enting behavior have demonstrated their ability to attend selectively to a novel comparison object that differs from a previously presented sample (e.g., Cohen & Strauss, 1979; but see Thompson, 1995). These early identity and oddity discriminations may serve as the basis for children's learning to name objects or events as "same" or "different." But their learning of those names is essentially no different from their learning to name "chair" or "red" (Skinner, 1957).

Naming our naming. Skinner (1957) made an important advance in proposing that, "Once verbal behavior has occurred and become one of the objects of the physical world, it can be described like any other object" (p. 319). This is the basis of his account of autoclitic behavior. It follows from our account of naming also that the objects that humans name include the naming responses themselves as well as the objects that first occasion those responses. Thus, for example, the caregiver may say to the child, "You're called Jo and she [pointing to another child] is also called Jo—you both have the same name." Following repetitions of such episodes, which may also often occur in relation to items that are physically similar, the child's verbal response "same" may at times come to be occasioned whenever she emits the same name for two or more objects. Just how different the name relation "same" is from supposedly analogous behavior established in nonverbal organisms is seen in work reviewed by Thompson (1995). He argues that the test of whether responding is truly relational and completely free from control by absolute cues is that subjects should be able to match stimuli in a match-to-sample task when the only basis for matching is the relation between two elements of a sample stimulus compound (e.g., AA) and the relation between another two elements within each of the comparison stimuli (e.g., BB vs. CD). If subjects can learn to respond to the sameness of the relation that exists in Sample AA and Comparison BB, they should then be able to learn to respond correctly with novel samples (e.g., FF) and comparisons (e.g., GG as correct and KN as incorrect). Similarly, if presented with KM as sample, the subject should select the correct comparison PQ and not the incorrect comparison, SS. Oden, Thompson, and Premack (1990) found that even chimpanzees that

showed evidence of generalized identity and oddity matching failed on such a task "despite heroic training efforts" on the part of the experimenters (p. 211). Whatever the basis of the chimps' discriminative responding in generalized identity tasks, these animals were apparently unable to *respond to that responding* as a way of performing correctly on this relational task.

There are, however, a variety of ways in which linguistically competent humans could pass Thompson's acid test of "truly relational" responding. One of the ways may be illustrated as follows: In an identity matching task, the sample AA may evoke in a 5-year-old child the basic level naming response "same," as may also the comparison BB. That is, when she looks at the sample and correct comparison, a child may say "same" and "same" in turn, or "that is the same" and "that is the same." But because the child can respond to her names as objects, she may now respond to these two consecutive "same" responses with the second-order naming response "same," listening to which she may now orient again to the sample and correct comparison (see Horne & Lowe, 1996). Reinforcement for selecting BB would also strengthen the naming of AA and BB "same" responses as "same" and ensure that this latter second-order name response would become discriminative for selecting the correct comparison in the future. In similar fashion, after looking at the sample AA and saying "same" and then the incorrect comparison CD and saying "different" the child, responding to these consecutive basic level names "same" and "different" as objects, may then emit the second-order name "different." Novel configurations (e.g., FF as sample, GG and HJ as comparisons) could, in subsequent tests, easily be encompassed by the child's naming "same" and "different" at both basic and second-order response levels. And in an identity matching task in which the stimulus components of the sample are different (e.g., where KM is the sample and PQ is the correct, and SS is the incorrect, comparison), the child may respond to herself saying "different" "different" to the sample and correct comparison, respectively, with the second-order name "same," and thus achieve success on the task. A similar account could also be

applicable to her performance on comparable oddity matching tests.

A human subject's naming of name relations is thus one of the ways that could ensure his or her successful performance on *abstract identity* tasks. Other verbal "strategies" (e.g., intraverbal naming) may be equally successful. However, that it is certainly verbal behavior of some sophistication that is necessary for such success (and see Thompson, 1995) is indicated by the fact that children do not generally succeed on tasks of this kind until they are about 5 years of age (House, Brown, & Scott, 1974; Lowenkron & Colvin, 1992).

Naming "more" and "less." As is the case for a child's naming of "same" and "different," learning to name "more" and "less" occurs over a long period. "More," although it is one of the first utterances of the young child, initially functions simply as a mand (e.g., as in "more milk," "more toys") (Bloom, 1970; Brown, 1973; Weiner, 1974). Gathercole (1979, and see 1985) has reviewed 24 studies concerned with the development of appropriate responding to "more" and "less." According to the findings of these studies, it appears that young children show a general nonlinguistic response bias in favor of adding items to a stimulus array, or of choosing the greater of two or more stimulus arrays. This early preference, of course, is likely to provide good opportunities for teaching a young child the name "more." In simultaneous discrimination tasks in which they are presented with two unequal stimuli and are asked "which one has more?" young children initially learn to respond to "more" correctly only within one particular stimulus dimension at a time. For example, the child's learning to respond to the longer of any pair of stimuli does not entail that she will also be able to respond to "more" correctly in relation to stimuli differing in terms of, for example, number, volume, or area. However, Gathercole's review shows that by the time they are 5 years old, children have generally learned to respond to "more" correctly across a range of dimensions. Their responding with comparable accuracy to "less," on the other hand, lags a long way behind, often by more than 2 years (see Palermo, 1973, 1974). This basic asymmetry in responding correctly to "more" and "less" argues strongly against any account of such behavior in

terms of *mutual entailment*; to the young child, being able to correctly select a stimulus that has "more" entails nothing about selection of the alternative stimulus as "less." These developmental data show that even for nonarbitrary stimulus dimensions, Hayes' account of the relational behavior of "moring" and "lessing" is unconvincing. The evidence rather is that children first learn a unipolar name relation, "more," in which a great variety of stimulus dimensions (e.g., length, number, volume, area, brightness, loudness) evoke the name "more." Once they have learned this relation, it is possible for the verbal community to instruct them to emit "less" in response to the stimulus that is the alternative to that named "more." This then enables children to respond appropriately to "less" across a range of novel contexts and stimulus dimensions for which they had previously learned to respond correctly only to "more."

Nonarbitrary transitive responding. Once children have learned to name "more" and "less" appropriately, do they go on to make *transitive inferences* about relations among a series of objects that vary in their properties or dimensions? For example, according to relational frame theory, a child who has seen pairwise combinations of different colored sticks that vary in length (e.g., Stick A > B, B > C, C > D, D > E) should, on the basis of combinatorial mutual entailment and without the opportunity for direct visual comparison, be able to infer the relation between novel pairwise combinations (e.g., BD) correctly and so say which one is longer than the other. However, this prediction is not confirmed. In studies conducted by Chapman and Lindenberg (1988), for example, it was found that only 5% of 7- to 9-year-old children responded correctly on such tasks. Clearly, even with real-world objects and late into childhood, children show little evidence of any facility with relational responding that has the property of combinatorial mutual entailment.

Making Sense of Relational Frame Research

Hayes and Barnes have chided us for not responding adequately to Hayes' (1996) challenge that we "explain the Steele and Hayes data." Because it provides yet another opportunity to contrast the respective explanatory merits of the relational frame theory and naming accounts, we shall here consider

some of the core features of relational frame experiments and, in particular, the study by Steele and Hayes (1991).

Establishment of contextual cues. A central characteristic of the procedures used in these relational frame experiments (see also Roche & Barnes, 1996) is that they aim to establish nonlinguistic stimuli as contextual cues for responding in accordance with such arbitrarily applicable relations as "larger" and "smaller." This is done by reinforcing subjects' selection of, for example, the larger of two or more stimuli, the greater number, the longer, and so forth, in the presence of putatively "nonlinguistic" contextual cues. A similar procedure is employed for establishing cues relating to "same," "different," or "opposite." But after such training can it be assumed that these cues are nonlinguistic? All the subjects in these relational frame studies are linguistically competent and, like R. Saunders (1996), we consider it far more likely that the cues come to be incorporated within the subjects' existing name relations of "more," "less," "same," "different," and "opposite" as occasioned by particular stimulus configurations in the experiment. This raises the further question of why the researchers do not explicitly name the cues (e.g., as "more," "less," etc.) in the first instance (see R. Saunders, 1996). This has, indeed, been recognized as a possible strategy by Steele and Hayes (and see Barnes & Roche, 1996). But, we maintain, if names were indeed provided it would make explicit what is already implicit in these procedures as presently conducted; that is, that the contextual cues derive their functional control from preexisting naming repertoires. Given that with verbally sophisticated subjects one cannot be sure that one has taught any new behavioral relations by means of such cue-establishing procedures, for the relational frame theory to be at all convincing these procedures need to be conducted with nonverbal subjects including young children. In such circumstances, however, we would confidently predict failure on tests of derived relations.

Testing-induced framing. Another feature of these procedures is that testing for derived relations is continued until the subjects "succeed"; that is, until they respond in accordance with the experimenter-defined stimu-

lus classes. This means that many test sessions are often required to establish each derived relation—up to 35 sessions in the case of the Steele and Hayes' (1991) study. We have already argued, as have R. Saunders and Green (1996), that such testing-until-success strategies, when used with verbally sophisticated subjects, cannot but provide implicit instructions and reinforcement for responding that conforms to the experimenter-defined classes. On these grounds also, therefore, it seems unwise not to take account of verbal control factors when interpreting the findings of relational frame studies.

Difficulty in learning relational framing. According to Hayes and Barnes, the relevant arbitrarily applicable modes of responding relationally ("moring," "lessing," "oppositing," and "differencing") are established early in childhood. This should indeed be the case if, as relational frame theory suggests, they are basic verbal processes. One would therefore expect well-educated adolescents and adults to have little difficulty performing appropriately on relational frame procedures. The reality, however, is that it takes a very long time and much effort to establish such performances and, even once established, correct responding is fragile. For example, in Dymond and Barnes (1995, Experiment 2) the 2 undergraduate subjects needed up to 800 trials to reach the first criterion for baseline relations. Both initially failed the tests for the derived relations. Only after an extended oscillation between being retrained in baseline relations followed by failing tests of derived relations did both eventually succeed; one of them required 1,280 baseline training trials before reaching the derived relation criterion. Relational frame theory cannot account either for these failures or for why, if they are supposed to be verbal processes, these relations should be so extremely difficult to establish. (Even 2-year-olds can learn entirely new name relations within a few presentations of a novel arbitrary stimulus and a novel object; see Baldwin, 1991; Nelson & Bonvillian, 1973.) We agree with Barnes and Roche's (1996) comment about such studies: "If the relational pretraining did not readily produce arbitrarily applicable relational responding in verbally sophisticated adults, RFT would be in very serious trouble as an account of human verbal behavior" (p. 492).

Why are stimuli within a frame of coordination not equally substitutable? Although the data are not discussed, Steele and Hayes (1991) recorded subjects' reaction times on test trials. There was a strong positive correlation between duration of reaction time and the complexity of the conditional stimulus relations tested in the probes (see also Wulfert & Hayes, 1988). But given that the stimuli within a frame of coordination (i.e., equivalence) are equally substitutable, then why should subjects take longer to respond on symmetry than on combined symmetry and transitivity trials (see also Spencer & Chase, 1996)? Indeed, if measures of response accuracy and latency do not covary on tests of frames of coordination or equivalence, then which, if any, is the "true" measure of these concepts? These are central questions which, to date, have not been answered by either Hayes or Sidman, nor have they accounted for the finding of Steele and Hayes and other studies that responding to supposedly equivalent stimuli differs systematically as a function of nodal distance or complexity. Such results are, however, readily explained within a naming account (see Horne & Lowe, 1996, pp. 235, 237; Lowe & Horne, 1996, p. 333).

Rule-governed framing. The performance of 1 subject in the Steele and Hayes (1991) study failed to satisfy the relational framing criteria. This subject's verbal reports clearly indicated that he had formulated rules for responding that ran counter to those required for "successful" entailment of relations established in baseline. Although he was the only subject for whom Steele and Hayes provide verbal reports, his account also supports our original suggestion (Lowe & Horne, 1996, p. 333) that much of the behavior observed in this study, including that which conformed to relational framing tests, was controlled by subjects' verbal formulations and rules. But this raises the question of whether there are two kinds of equivalence performance—one that is rule governed and one that is contingency shaped—and, if so, how these different kinds of equivalence come about. Neither relational frame theory nor Sidman's theory has addressed this issue, which is central to behavior analysis.

Conclusion

We find the objectives of all these experiments on relational framing, including the

new framing procedures outlined by Hayes and Barnes, to be obscure. As R. Saunders (1996) has observed, "That newly trained symbolic substitutes for the words *same*, *opposite*, *different* control responding to pairs of stimuli . . . is not unexpected; elementary school children perform this feat daily in the classroom" (p. 486). And it is perplexing that in devising their studies the relational frame theory researchers should go to such lengths to avoid the inclusion of explicit verbal cues as to render it very difficult indeed for subjects to perform the feats required of them. What such language-avoidance research with humans could possibly be expected to tell us about language, logic, or learning in general is most puzzling of all.

With regard to relational frame theory itself, we can see little to recommend it. It seems to have many of the flaws of the equivalence account upon which it is based, but it extends and amplifies some of the most problematic features of the original. Instead of just one central abstraction—equivalence—there are several different kinds of frames that intervene between behavior and the environment, described in only the most abstract of terms. The considerable gulf that exists between this theory and the empirical evidence on relational learning in children only further undermines its plausibility.

DO NONVERBAL ANIMALS PASS EQUIVALENCE TESTS?

Schusterman, Kastak, and Reichmuth's commentary indicates that they have misunderstood our views on the relation between thought and language. We do not assume that thought is dependent solely on words or that "the word is the sole sign of thought" (p. 252). As we indicated in our original paper, our views on this issue are closely aligned with those of Skinner, Vygotsky, and Mead, none of whom provided what could be mistaken for Cartesian or non-Darwinian accounts. We recognize that there must be a clear continuity of those behavioral processes that might be termed *thinking* from nonhuman to human animals, but we also believe that there are discontinuities, particularly insofar as language affects human behavior (Lowe, 1983; Lowe & Horne, 1985; Lowe,

Horne, & Higson, 1987). Like Skinner, Mead, and Vygotsky, we propose that the form of behavior that we term *verbal thinking* (Horne & Lowe, 1996) is unique to the human species, and unsurprisingly, perhaps, we consider language to be a *sine qua non* for that behavior. We have consistently maintained that to ignore this qualitative difference is to miss a central aspect of human behavior, as well as some of the central tenets of radical behaviorism.

Also contrary to Schusterman et al.'s understanding of our position, we have not tried to account for equivalence; given that the concept seems to us to lack coherence it makes little sense that we should try to explain it. Our interests in this domain focus instead mainly upon the symmetry and combined symmetry and transitivity behavioral relations that are commonly observed in human performance in match-to-sample procedures. And our concern here is with whether language is necessary to generate such performances. Our position is that we can see how verbally competent humans could pass these tests, but we accept that animals might also succeed. If they did, however, we would need to know how, because, assuming that the test procedures were free of artifacts, we would maintain that such behavior could not be predicted from animal learning principles as presently known. Whatever findings finally emerge, our main concern is that success on these tasks should not be confused with language itself.

The Schusterman and Kastak (1993) study. As a possible demonstration of stimulus equivalence in animals, many would argue that the Schusterman and Kastak (1993) study of a sea lion, Rio, has been the only real contender (see Lowe & Horne, 1996, p. 331). Clearly, Schusterman et al. are in little doubt about its validity. But, particularly in the light of new experimental evidence, we are highly skeptical of this conclusion. It is important to reconsider in some detail the procedure that was used, not least because many of its features are repeated in the new studies of supposedly derived relations outlined in their commentary. In Figure 1 we have attempted to illustrate the stimulus configurations seen by Rio on training and test trials. For purposes of illustration we consider only one of the 30 stimulus sets, Set 16, but the same analysis

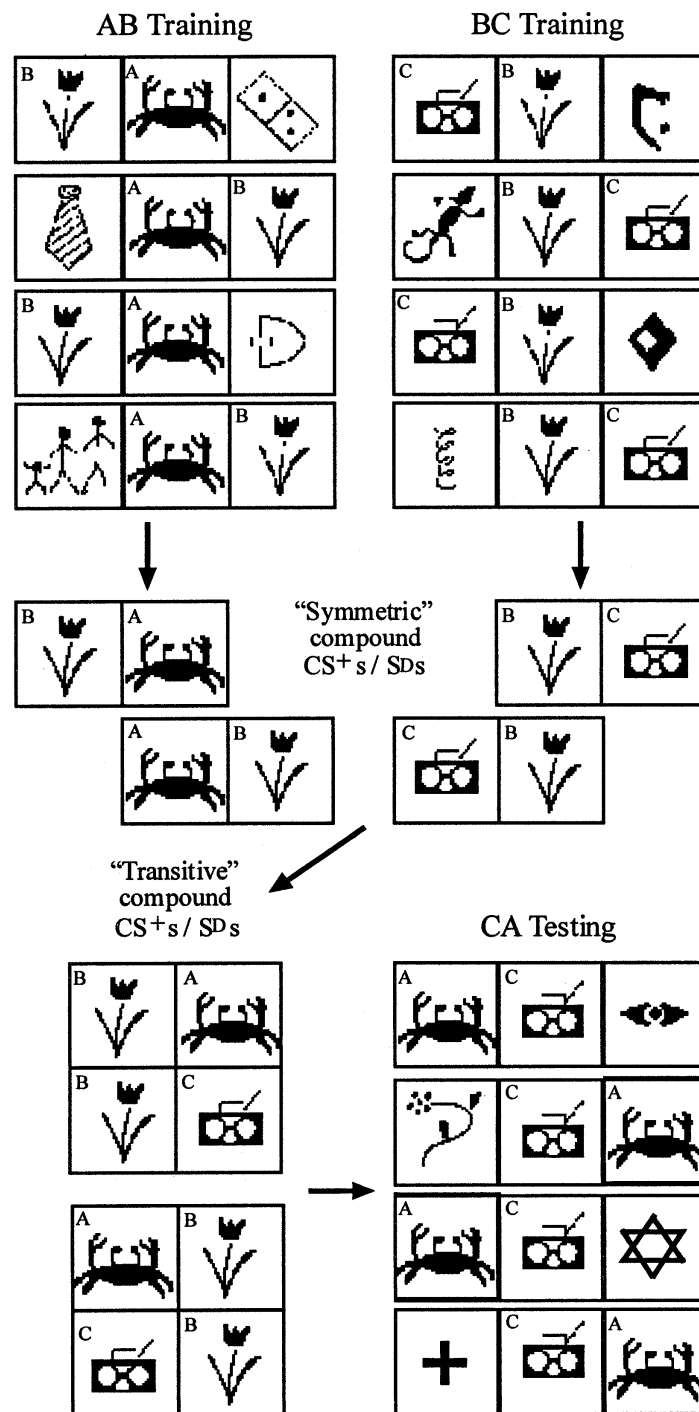


Fig. 1. Examples of the stimulus configurations seen by the sea lion in the Schusterman and Kastak (1993) study. This shows stimuli from one of the 30 stimulus sets (Set 16, labeled here A, B, and C) and a selection of the negative comparison stimuli, which are not labeled. The top left panel presents four of the 29 three-stimulus arrays used during AB training; comparable arrays for BC training are shown in the top right panel. The center panels show the "symmetric" compound stimuli, consisting of AB/BA pairs (left) and BC/CB pairs (right) that may have been established during training. The lower left panel reorders these compound stimuli to show how "transitive" stimulus compounds may also have been established. Examples of the stimulus arrays presented in CA testing are shown in the lower right panel.

applies to the remaining 29 sets. Figure 1 shows Set 16 stimuli (labeled A, B, and C) and a selection of the negative comparison stimuli (not labeled) that were presented during training and testing. The top left panel shows four of the 29 three-stimulus arrays used during AB training, in each of which the correct comparison, B, consistently appeared either on the left or on the right of Sample A. Note that the incorrect comparison stimuli were never the same from trial to trial. During AB training, Rio was not required to approach or otherwise respond to the sample, so each trial essentially consisted of the presentation of a three-element array, two elements of which were consistently paired while the third was consistently varied. Thus, when Rio's responding to B was reinforced this always occurred in the presence of the adjacent A and of no other reliable accompaniment. The same was true for BC training, as shown in the top right panel.

Schusterman et al. might wish to consider how similar their procedure is to that employed in a recent series of studies by Siemann, Delius, Dombrowski, and Daniel (1996). In the first of these experiments, pigeons' responding on four successively presented target stimuli was reinforced with graded amounts of reward and penalty (A++, B+, C-, D--). These target stimuli were accompanied by four different neutral stimuli (Na, Nb, Nc, Nd); thus, Na accompanied A++, Nb accompanied B+, Nc accompanied C-, and Nd accompanied D--. Responses to the neutral stimuli had no consequences. After discrimination of the target stimuli had been established, tests of the neutral stimuli revealed a graded preference increasing from Nd to Na. A second experiment used two target stimuli, A+ and B-, accompanied by two neutral stimuli, Na and Nb, respectively. The pigeons developed a very strong preference for Na over Nb, ranging from 92% to 100% in unreinforced testing. To ensure that these effects were not due to adventitious reinforcement of operant responding on the neutral stimuli, a third experiment showed similar strong effects even when the opportunity to respond on the neutral stimuli during training was removed. If one considers the Schusterman and Kastak study in the light of this evidence (see also Wynne, 1995, 1997), one observes that on all

Rio's AB training trials the centrally positioned Stimulus A was, effectively, a neutral stimulus accompanying the outer situated Stimulus B on which responding was reinforced. Even though there were never any scheduled consequences for responding to A, following Siemann et al., we would expect that if Rio were provided with the opportunity to respond on A, that is, if A were situated in either of the outer positions (responding to the central location was never reinforced), then the animal would do so. Because on other trials A was presented with other stimulus pairs as an incorrect comparison, responding would occur on A only in the presence of B. Thus, by the end of training, the stimulus configurations AB (i.e., where B is to the right of A) or BA (i.e., where B is to the left of A) would be discriminative for responding on either B or A, depending on which occurred in the outer location. The same analysis applies to control by the BC/CB stimuli. It follows that if Rio were tested for symmetry with these stimuli and B were presented in the center with A as the "correct" outer stimulus, then, again following Siemann et al., we would expect responding to be directed at A. This would yield success on any symmetry test BA. The same would be true for any CB symmetry test following BC training.

The argument that the AB pair also functioned as a compound Pavlovian conditioned stimulus (CS+; see center panels of Figure 1) is based on the observation that AB was reliably followed by delivery of food and so should have become a strong appetitive CS+. The same was true of BC. Because B was common to both the AB/BA and BC/CB compound stimuli, it is reasonable to assume that by the end of the AB and BC baseline training, any combination of A, B, or C (see lower left panels of Figure 1) would have functioned as a compound appetitive CS+. Given that A shared both the discriminative and CS+ functions of B and that B shared the discriminative and CS+ functions of C, it follows that whichever two of these three stimuli were present, the animal should respond to the outer element. Consequently, in equivalence testing (see Figure 1, bottom right panel), when presented with the AC/CA compound Rio should respond to its outer element, A, and not to the other stimulus,

which would never previously have been presented with AC or CA. Thus, our conclusion is that simpler conditioning principles than equivalence can account for Rio's performance (and see Horne & Lowe, 1996, pp. 223–224; Lowe & Horne, 1996, pp. 330–331).

Other sea lion studies. We here briefly consider the previously unpublished experiments to which Schusterman et al. refer, although in the case of these, even more than for the earlier study, we lack important procedural information. The first of these experiments was designed to investigate transfer of discriminative function within the three-element stimulus sets employed by Schusterman and Kastak (1993). We believe that our previous interpretation of that study, showing as it does how stimulus compounding and transfer of discriminative control across elements within the compound were likely to have occurred, readily accounts also for the outcome of this transfer experiment, thus obviating any need to appeal to equivalence as an explanation.

In their second experiment, which had as subjects 2 sea lions, Rio and Rocky, the experimenters' aim was to establish functional classes using contingency-reversal procedures (Sidman, 1994, pp. 451–453). Rio was then tested for the emergence of equivalence between functional class members. Although it is difficult to determine whether they apply to this particular study, Sidman (1994) has made some interesting observations about across-trial discrimination learning during such procedures to the effect that when simultaneous discrimination performance approaches criterion levels, then from one trial to the next, Set 1 stimuli will invariably be selected and reinforced, and thus conditional sequences between Set 1 members may be reinforced. Moreover, because the order of presentation of Set 1 stimuli is randomly varied, all possible conditional relations between Set 1 members can be learned in this way. When the contingency is reversed, and selection of Set 2 stimuli in the simultaneous discrimination task is reinforced, all possible relations between Set 2 stimuli can be learned in similar fashion. With each reversal, such adventitious conditional discrimination learning between set members would be strengthened. In this experiment conducted by Schusterman there were, for each set, 90 possible con-

ditional relations to be learned. Each session consisted of 40 randomized trials, so Rio had ample opportunity to learn these across-trial relations for both stimulus sets. If correct, Sidman's interpretation would account both for the establishment of the functional classes that occurred in this study, and, because all of the stimulus relations had already been trained, for Rio's above-chance match-to-sample performance.

The final phase of this experiment again concerned only Rio. Bidirectional relations were trained between a new stimulus, K, and one of the Set 1 stimuli, J, and between a new stimulus, 11, and one of the Set 2 stimuli, 10. When Rio was tested for bidirectional conditional relations between K and all other Set 1 members, and between 11 and all other Set 2 members, she passed all of the tests. However, given, as has already been suggested, that all of the bidirectional relations between stimuli in each of the two sets had already been trained in the previous two phases, then, in training the bidirectional link between K and J, the K stimulus should become transitively related to all of the Set 1 stimuli. The JK compound trained during this phase would have an element, J, in common with the stimulus compounds trained in the previous phase (i.e., AJ, BJ, CJ, DJ, and so on). These are just the conditions that we have already argued are sufficient to establish relations between A and K, and indeed between K and all other Set 1 stimuli. The same analysis applies to the relation of Stimulus 11 to Set 2 stimuli.

As Sidman has ably demonstrated on many occasions (see Sidman, 1994), knowledge of the procedural details of match-to-sample experiments, particularly those conducted with nonhuman animals, is crucial for accurate interpretation of data. In the absence of such information about these experiments, any interpretations of the findings, including our own, must remain provisional. Furthermore, in all but one of them Rio alone participated as subject; for some reason neither Rocky nor any other sea lion featured in any of the crucial tests. We would also need to know more about the role of the set-specific reinforcers (a different type of fish was used for each set) that were provided in three of the four experimental procedures; one would need to be certain that the possibility of inadvertent

cues (e.g., olfactory) was excluded. If there were such cues, of course, there could be an even simpler interpretation of the findings than the one we have offered here. Such issues, however, can be straightforwardly resolved by the provision of more detailed information, and what is most important, by replication of the effects using other subjects and appropriate control procedures.

Equivalence in other species. Whatever the case with Schusterman's own experimental work, there is clearly a problem of overinterpretation of data in his discussion of other animal literature. In the general quest for "derived" relations that has taken place over recent years, one of the distinctive merits of Sidman's approach has been the care he has taken to ensure that the stimulus relations under investigation could not have been established by preexperimental histories, ontogenetic or phylogenetic. Given the least rigor in this regard and a basic application of Lloyd Morgan's (1894) canon, it seems that none of Schusterman's examples of equivalence in the behavior of animals in natural social settings could pass muster. First, the example of cross-modal equivalence in pinnipeds can be explained more simply in terms of behavior being occasioned by multisensory stimulus compounds; there is, accordingly, no emergent behavior to account for. Second, the notion that when vervet monkeys hear an absent juvenile's scream and then look at its mother, there exists an equivalence relation between the scream, the juvenile, and its mother, might have some credibility if the juvenile were never seen in the company of its mother and she never responded to its screams. But this seems unlikely. Similarly, there are no grounds for invoking equivalence to explain group aggression in vervet monkeys. Also, vervet monkeys' responding to acoustically different calls undoubtedly has a major phylogenetic component. Once again, there is no basis for invoking derived relations such as equivalence in this case. Indeed, in his review of the literature, Thompson (1995), cited by Schusterman, says of overinterpretations of these alarm call studies, that "The processes need be no more complex than those involved in associative conditioning" (p. 207). Finally, appealing to equivalence as a factor in monkeys' social relationships (Dasser, 1988) is equally unfounded, because, as

Thompson (1995) has observed, such behavior is almost certainly "mediated by common functional associations such as temporal/spatial proximity and interactive outcomes" (p. 206).

Do We Need a Molar Account of Behavior?

Tonneau and Sokolowski consider our analysis of stimulus equivalence to be largely correct. However, in their own contribution they reveal some fundamental misconceptions about our account as well as, it would seem, about some of the basic phenomena that we deal with. To take a minor issue first, we do not attribute the failure of equivalence explanations to the use of match-to-sample procedures. Our point is simply that if the main goal is to understand how language is learned and has its effects on other behavior, then one should not be bound by a rigid adherence to match-to-sample procedures. It is also not the case that the core of the naming account lies in intraverbal behavior. On the contrary, our basic unit of verbal behavior is the name relation. Our account accordingly pays particular attention to the powerful and pervasive role of *common naming* in the classifying of objects and events, both in the context of match-to-sample and other categorization studies and in the world at large. But we have also been concerned to show that names evoke names (as in intraverbal behavior) and that names themselves can be named (as in autoclitic behavior) and that, between them, intraverbal and autoclitic behavior can give rise to an almost infinite variety of rules for responding in match-to-sample tasks. For example, the reversibility inherent in intraverbal name sequences (see Horne & Lowe, 1996) may become the occasion for autoclitic responses such as "green means up" (see above and Skinner, 1957). All these variants of verbal behavior are, however, basically founded upon the name relation functioning either as a common name or as individual names for the stimuli concerned.

Of more importance for Tonneau and Sokolowski's own thesis is their mistaken belief that we maintain that "the behavior of non-verbal animals should . . . conform to standard molecular Pavlovian and operant laws" (p. 267). The problematic words here are "standard" and "molecular." We do not have a rigid commitment to well-established, as op-

posed to novel or "nonstandard," accounts; nor have we any particular prejudice in favor of molecular over molar accounts of behavior. Indeed, in our treatment of the animal literature in this area (see above) we have tried to incorporate some of the recent research and thinking about new behavioral phenomena, including the complexities that result from interactions between operant and Pavlovian conditioning. We have tried to reflect these complexities in our account of the name relation itself (see Horne & Lowe, 1996).

We have already dealt with some of the misunderstandings concerning whether ours is a mediational account (see Naming and Mediation, above). We also need to point out, however, that Tonneau and Sokolowski's enthusiasm for a languageless molar account, which they illustrate with Holland's (1990) mediational theory, appears to be founded upon a mistake as to what constitute the behavioral criteria commonly taken to indicate equivalence. They assume that having trained AB and AC to meet the criteria for stimulus equivalence, it is sufficient to show that BC has emerged. But they thus miss what is perhaps the central imperative in this research area, namely the necessity of demonstrating symmetry. To demonstrate that the relations are symmetric rather than merely transitive, it is essential to show not merely the emergence of BC but also of CB, BA, and CA. It is these symmetrical relations that are the most elusive in the animal literature. And given that existing learning principles, even bolstered by Holland's (1981, 1990) mediational theory, cannot account for them, this should come as no surprise. It should be noted, however, that a molar explanatory system based on Holland's model would be unable to account for the phenomena that seem to be of central concern, that is, emergent relations on tests of stimulus equivalence. From AB and AC trained relations, Holland's model yields BC but not CB, BA, or CA. (To be fair to Holland, he does not claim that his model accounts for all of these emergent relations.) Invoking other studies that have appeared to show backward conditioning effects (albeit small and fragile), usually with food or some other unconditioned stimulus as one of the stimulus elements, does not counter the evidence that, as yet, there has been no reliable

demonstration of even these simple symmetric relations in animals other than humans. We propose, of course, that it is the bidirectionality inherent in verbal relations such as naming that enables humans to readily produce bidirectional performances in match-to-sample tests.

To conclude, (a) it is not possible to extend the naming account to the behavior of other animal species because, so far, it has not been shown that they can learn to name objects and events, and (b) if by a molar account, Tonneau and Sokolowski mean a return to a simple methodological behaviorism in which the complexities of verbal behavior are ignored, and even simple phenomena like symmetry cannot be explained, then we are certainly less than enthusiastic about its advantages.

OTHER ISSUES

How is naming established? Lowenkron alone among the present commentators does not address equivalence. Instead he focuses upon what we ourselves regard as a much more important issue, that is, how the basic unit of verbal behavior that we term *naming* is established. Although in all other respects he seems to be in accord with our approach, he does perceive problems in our description of the name relation. He thus proposes an additional factor—*joint control*—that is a behavioral process supplementary to those we describe; the inclusion of this process he believes would deal with the apparent difficulties in our account and would thus enhance its plausibility and generality. We are grateful to Lowenkron for his careful scrutiny of the detail of our explanations; part of the purpose of this project was certainly to identify problems and solutions where they existed. However, in this particular case, we do not see that there is a problem to be accounted for, nor do we see the need for insertion of another behavioral process. Indeed, in our view, Lowenkron's joint control "solution" would introduce insoluble problems.

As Lowenkron understands it (see Lowenkron, 1996), the difficulty lies in that part of our account where, the basic name relation having been established, a new object is presented to the child and is named by the caregiver; the child not only names this object but

also emits appropriate listener behavior in response to the utterance (i.e., looks at the object or reaches for it). Not being able to identify any source of differential reinforcement for such listener behavior in this context, Lowenkron is puzzled as to how it comes to be established when only the tact relation (i.e., object-utterance) has been trained. In other words, if one trains only speaker behavior, how does listener behavior emerge? Interestingly, the converse process, where listener behavior to a name is trained but speaker behavior (i.e., the tact component) emerges, is not viewed by Lowenkron as a problem (see also Michael, 1996).

In view of Lowenkron's return to this topic, it seems that we did not devote sufficient space in our last reply to dealing with it. It is, we believe, an entirely straightforward matter. In our account we describe how, prior to training any verbal productions, caregivers establish an extensive repertoire of listener behavior in the young child and then an echoic repertoire. Naming is established by the caregiver (a) pointing to (or showing) and looking at the object to be named, (b) saying the name of the object, (c) asking the child to echo the name, and (d) reinforcing appropriate listener behavior and verbal productions (e.g., saying the word while looking at the object). The vital feature here is that both the child's speaker behavior (i.e., her utterance of the word) and her listener behavior (i.e., her looking at or reaching for the object) are differentially reinforced; saying, without looking at the object, is not normally reinforced, and neither is looking at the object without saying. Many names are established in this manner, so that the child is provided with an extensive history of reinforcement for appropriately conjoined saying and looking. Most initial names are based upon existing listener behavior repertoires, with speaker behavior lagging well behind comprehension up to and beyond 2 years of age (Baldwin, 1991; Benedict, 1979).

Now we come to the seat of Lowenkron's problem, which he locates in what he terms object-name, or tact, training. Let us look closely at what actually occurs in the course of such training, for example, when a new object (a bell, say) is introduced to the child. The caregiver says, "This is a bell. Can you say 'bell'? What is it?" The child's previous

name training (see above) will have established that she behave along the following lines: (a) Upon hearing the novel verbal stimulus /bell/, she orients first to the caregiver and then to the bell that the caregiver is looking at. This listener responding, because of the extensive listener repertoire she has already acquired, occurs very reliably, often even in the absence of explicit reinforcement (Baldwin, 1991). (b) As she looks at the bell, she says "bell." This part of the sequence establishes the discriminative control of the bell over her saying "bell" where previously it was only an echoic. The caregiver then says "Good girl! Yes, it's a bell!" and thus reinforces the entire behavioral sequence, from her hearing the name stimulus, to seeing the object, to echoing while looking at the object, and once more hearing the name. The problem Lowenkron perceives (namely, how one accounts for listener behavior when the caregiver apparently only trains the tact or object-name relation) thus in reality simply does not exist. In reinforcing the would-be tact, caregivers at one and the same time reinforce both echoic behavior and appropriate listener behavior. As we have previously argued (Horne & Lowe, 1996, p. 200), tacts are a rare feature in child behavior precisely because, in the normal course of events, as speaker behavior is trained, listener behavior is simultaneously acquired, and both function together as the name relation.

There is thus, in our view, no hiatus in the account that joint control need fill. In any case, as we understand it, the notion would require that, for each name relation, caregivers should train first echoic responding and then the corresponding tact relation, but not any listener behavior. This is highly unlikely in the real world of infants' language learning. Furthermore, it is clearly a redundant concept when applied to what is the primary route to naming, that is, where listener behavior is already established and all that is required is to reinforce echoing in the presence of the object. One has to say, also, that if language were dependent solely upon joint control as described by Lowenkron, it would be a poor thing indeed, for most of the stimulus categorization and semantic relations described in our account would have to be eliminated. As Lowenkron acknowledges, it would mean a return to the exclusively speaker di-

mension of Skinner's *Verbal Behavior* (1957) and an abandonment of any aspirations towards understanding speaker-listener effects. If joint control were the principal determinant of verbal behavior, how would one respond to the utterance "The stars are bright tonight"? The speaker here has not instructed the listener to find bright stars, so no particular listener behavior (e.g., looking up at the stars) should be occasioned. But even if this utterance were somehow to occasion "search and find" behavior in the manner described by Lowenkron, it would then be necessary for the listener to continually echo "bright stars" while scanning the environment until stars were encountered and evoked the tact response "stars"; the conjunction of the tact and the echoic responses would then in turn evoke the listener behavior of looking at and, perhaps, pointing to the stars. But we maintain that all of these steps are unnecessary: An auditory stimulus such as /stars/ alone is sufficient to occasion the conventional listener behavior of looking up at the stars. Furthermore, according to our understanding of Lowenkron's account, speakers would be able to listen to or comprehend only those of their own verbal productions with which they had first instructed themselves to find one or more objects that occasioned particular tacts, which would mean that they could not understand most of their own verbal productions. It has to be said that this is not a linguistic environment we recognize.

Lowenkron's notion of joint control is based upon research he has conducted with adults and children, individuals all much older than those who are learning their first words and who are the main focus of our account. But, like others, he has challenged us to account for his findings. Unfortunately, his own theoretical interpretations of these findings are based upon the view that linguistically competent individuals tact rather than name objects and events in their environment. We have tried to show here that this does not happen. Children learn bidirectional name relations, not tacts. An account that combines a description of the autoclitic behavior involved in following simple instructions (and see Relational Frame Theory and Naming, above) with naming relations should have little difficulty in explaining most of

Lowenkron's data. We would be happy to pursue such an analysis with him.

Learning from history. Moerk thinks he perceives a fault line running through our project at the join between a highly artificial paradigm (match to sample) and the "immense problem area of naturalistic meaning and reference" (p. 248). Because our account is not based upon that particular paradigm, it appears that he has misconstrued our objectives. Indeed, we have argued that theoretical and empirical research in behavior analysis should be redirected away from stimulus equivalence studies and should focus instead directly upon verbal behavior itself and its role in categorization and other complex human behavior.

Moerk's astonishment that stimulus equivalence has not been established in animals appears to arise from a misconception about what is entailed by that concept. For whatever the confusions that surround equivalence, it is clear at least that symmetry is an essential element of it. But symmetry is just what is missing from all of the examples Moerk cites from observations of animal behavior and children's semiotic development. Although it may be the case, for example, that a gazelle's responses to stimuli that are discriminative for predators do resemble, in some respects, an infant's listener behavior to "Where is the teddy bear?," neither example involves symmetry or, presumably, any "derived" behavior. Tales of cats pressing latches, dogs bringing leashes, and even vervet monkeys producing "signals" (see Do Nonverbal Animals Pass Equivalence Tests? above), are also far from convincing evidence; Lloyd Morgan (1894) is a good guide on such matters. And although it is true that there are examples of manding in studies of animal and infant behavior, manding must not be confused either with tacting or with the bidirectionality involved in naming and name manding (Horne & Lowe, 1996). Neither Whitehurst and Fischel (1994) nor any other study has reported evidence of symmetry in prelinguistic infants.

The work of Nelson and Clarke and the CHILDES network are indeed important sources of information for anyone concerned with language learning; they have certainly had an influence on our research. As for the other sources that Moerk cites, some simply do not make significant contact with the kind

of functional analysis of speaker-listener behavior we have attempted. However, we endorse his view that to engage in either empirical research or theory building in this domain while in ignorance of the related work that has already been done in, for example, philosophy, psycholinguistics, neuropsychology, or developmental psychology is to be doomed to repeat past errors and, what is worse, having made basic errors, not to know it.

Earlier work of relevance to our endeavor would certainly have to include Frege's logico-mathematical approach to semantic analysis, which Moerk rightly points out has similarities with that of equivalence theories, particularly in relation to the concept of *identity*. But why he should assume that we are not familiar with Frege's theory of semantics and have not considered it in our account is puzzling. A number of the papers we cite, including those by Ryle (1949b) and Hunter (1974), are explicitly devoted to the shortcomings of that theory, and these problems are now widely acknowledged. We cited such works precisely because it appeared to us that some of the mistakes made by Frege in the last century were being repeated by equivalence theorists today, albeit to a greater degree. Indeed, the work of Putnam (1981) and others, casting doubt on the very possibility of semantic analysis as conceived by Frege and his successors, serves as a useful caution against any new endeavor to understand meaning and reference on the basis of abstract logico-mathematical rules rather than upon an analysis of language as it is learned and used by human beings from infancy onwards. Scruton (1994) outlines what at least some philosophers now conclude:

Hitherto we have described the workings of language without reference to the context of use, as though words were attached to the world by stipulation, through arbitrary rules. But words do not come into use in that way. They are *taught* to us, by others who observe our circumstances from outside. I see the child staring at a horse, and say "horse"; but I am already assuming not merely that the horse is there, but that he *sees* the horse—i.e. that the horse causes in him a particular perceptual experience. This causal link between the world and the observer is built into the language. "Horse" comes to mean a certain

kind of thing, which acts on an observer in the way that horses do. A link between the speaker and the world is established in the very meaning of the word. (p. 264)

Reference and meaning are not, in other words, arbitrarily given but rather are learned in the course of human development. If this is indeed the case, as we believe it is, a developmental analysis of meaning and language such as we have undertaken is likely to prove to be the most helpful way of advancing understanding in this domain. As the experience with Frege's ideas indicates, however—and here we wholeheartedly concur with what must surely also be Moerk's more general point—all of us engaged in research in this area have much to learn from not only the successes but the failures of those who have previously grappled with these most difficult issues. Many questions, of course, remain to be answered, including those concerning the learnability of naming, but we must ask the right questions and in ways that make answers possible. Because the issues are so complex and inherently multidisciplinary, it is also vital that we conduct our inquiries and research in a manner that eases and encourages ongoing discourse with colleagues in other research traditions and disciplines. This has been one of the main objectives of our account.

CONCLUSION

In the 18th century, Kant described the problem of how anything in the human mind can be a representation of anything outside the mind as the most difficult riddle of philosophy. Putnam (1992) suggests that at the close of the 20th century that question has been replaced by "how does language hook into the world?" This is a burning issue not just for philosophy but for many disciplines concerned with the study of human behavior. For example, taking the form "how are 'symbols' grounded in the real world?" it continues to bedevil cognitive psychology (Harnad, 1990; Searle, 1980). And within behavior analysis, as we have seen, it has been the spur to our own work as well as to the enormous body of research on symbolic match to sample that is termed *stimulus equivalence*. For some time that latter work has seemed, in the view of many behavior analysts, likely to pro-

vide the key to understanding what it is for behavior to *symbolize* or *refer to* objects and events and to have *meaning*. As we have noted, the particular stimuli and examples it uses, often words or pictures, encourage the belief that symbols are indeed the focus of study here. After all, is it not the case that subjects in these experiments treat the stimuli, even when they are abstract shapes, with just that interchangeability which characterizes how people respond to words and referents? Accordingly, the proponents of this approach believe they have gone some way towards solving Kant's problem; the necessary and sufficient conditions for language and symbolic representation, they argue, are the behavioral criteria that together make up equivalence. Thus sidestepping a systematic study of the development of verbal behavior, they simply lay down a formal specification of what it is for behavior to be linguistic or symbolic, just as though, as Scruton (1994) says of the approach of Frege and his successors, "words were attached to the world by stipulation, through arbitrary rules" (p. 264). Such an approach has not worked in the past, and in the guise of equivalence theories it fares, if anything, even less well now.

For the sake of future work it is, however, important to draw lessons from the equivalence enterprise. It has highlighted a fundamental problem that perennially besets research in psychology. Skinner (1969) has described it as the formalistic fallacy, and it occurs when an undue emphasis is placed upon formal characteristics of behavior at the expense of controlling relations (and see Vygotsky, 1978, pp. 58–75). To take an analogy from another science, in its appearance and behavior a dolphin stands closer to the fish family than to the mammal, but biologically it is closer to a cow than to a shark. In the behavioral domain, two patterns of behavior (e.g., performances on a simple schedule of reinforcement, conformity to the matching law, or even equivalence) may be very similar, but a functional analysis may reveal that the determinants are very different and that one is contingency shaped and the other is rule governed (Horne & Lowe, 1993, 1996; Lowe, 1979). In studies of equivalence, the tests may show that subjects' behavior conforms to the criteria specified by set theory and, in the case of relational frame theory, other simple

logical entailments; human language has these characteristics, it is argued, so equivalence relations are verbal relations (Hayes, 1994; Sidman, 1994). But these assertions are made in the absence of a functional analysis of how any of the relations have come about or, indeed, of how verbal behavior in any form has been established. And whatever one's allegiance to mathematics and logic, behavior analysis surely should not assume that meaning, reference, and symbolic relations between words and the world are a given.

If we wish to build a proper theory of verbal behavior and how it relates to objects and events, then we need to begin with the study of prelinguistic behavior in infants (and, perhaps, other nonverbal organisms) and advance systematically, so that we are able to trace the determinants of each new component of verbal behavior as it arises. It would be very difficult indeed to arrive at such a theory if one had to rely only on information gleaned from studies with humans whose linguistic skills were already well established and in which the forms of responding under consideration had already occurred with such a countless frequency in the lifetime of each individual subject as to become what Vygotsky (1978, p. 63) termed "fossilized behavior." This is a clear shortcoming of the many equivalence and relational frame studies that investigate skills that, in practice, have been well established and rehearsed over several years (see K. Saunders & Spradlin, 1996, and Relational Frame Theory and Naming, above); here it might be said that the equivalence or framing outcomes have, in fact, been studied postmortem. When we have understood how verbal behavior is established and has its effects on other behavior in its relatively simple and early forms, we will be in a better position to take on the complexities of such interactions as they occur in adult match-to-sample performance. A key point here is that we can, in children, study conditional discrimination learning and other forms of categorization and discrimination before, during, and after the learning of core linguistic skills.

A proper theory of verbal behavior, we propose also, should not be founded on the mathematical and logical abstractions of verbally sophisticated adult humans (i.e., mathematicians and logicians) but on the study of

the basic relations of verbal behavior itself. Skinner's *Verbal Behavior* (1957) remains, we believe, an invaluable text in this respect, and we should build upon it. In the best traditions of scientific progress, however, where his account is found wanting it should be constructively confronted and, combined with the findings from empirical research, this should lead to the development of a new theory of verbal behavior. Such has been our objective in this project.

Of great importance, however, is that these new theoretical developments, which we hope have been advanced by the present exchange of ideas, should generate fresh approaches to empirical research, particularly on complex human behavior. The experimental program of our own laboratory in recent years, for example, has focused upon how naming is established in infancy and how it affects categorizing and other behavior. Studies include investigations of different forms of verbal behavior (such as vocalizing and manual signing) and their differing effects on transfer of function, and of the conditions necessary for bringing about naming itself. Progress in unraveling issues that, like these, lie at the heart of the development of verbal behavior will depend upon considerable experimental innovation and concerted research effort on the part of very many of us. Informed by a systematic empirical endeavor of this kind, could an account such as we outline here bring us closer to solving the problem of how language hooks into the world? It is the long and difficult behavioral route, but we know of no other that might succeed.

REFERENCES

- Baldwin, D. A. (1991). Infant contributions to the achievement of joint reference. *Child Development*, 62, 875–890.
- Barnes, D. (1994). Stimulus equivalence and relational frame theory. *The Psychological Record*, 44, 91–124.
- Barnes, D., & Roche, B. (1996). Relational frame theory and stimulus equivalence are fundamentally different: A reply to Saunders' commentary. *The Psychological Record*, 46, 489–507.
- Benedict, H. (1979). Early lexical development: Comprehension and production. *Journal of Child Language*, 6, 183–200.
- Bentall, R. P., Lowe, C. F., & Beasty, A. (1985). The role of verbal behavior in human learning II. Developmental differences. *Journal of the Experimental Analysis of Behavior*, 43, 165–181.
- Bloom, L. M. (1970). *One word at a time: The use of single word utterances before syntax*. The Hague: Mouton.
- Brown, R. (1973). *A first language: The early stages*. Cambridge, MA: Harvard University Press.
- Catania, A. C. (in press). *Learning* (4th ed.). Englewood Cliffs, NJ: Prentice Hall.
- Catania, A. C., Horne, P. J., & Lowe, C. F. (1989). Transfer of function across members of an equivalence class. *The Analysis of Verbal Behavior*, 7, 99–110.
- Catania, A. C., Matthews, B. A., & Shimoff, E. (1982). Instructed versus shaped human verbal behavior: Interactions with nonverbal responding. *Journal of the Experimental Analysis of Behavior*, 38, 233–248.
- Chapman, M., & Lindenberg, U. (1988). Functions, operations, and decalage in the development of transitivity. *Developmental Psychology*, 24, 542–551.
- Cohen, L. B., & Strauss, M. S. (1979). Concept acquisition in the human infant. *Child Development*, 50, 419–424.
- Dasser, V. (1988). A social concept in Java monkeys. *Animal Behavior*, 36, 225–230.
- de Rose, J. C. (1996). Naming, meaning, and verbal operants. *Journal of the Experimental Analysis of Behavior*, 65, 274–275.
- Dugdale, N., & Lowe, C. F. (1990). Naming and stimulus equivalence. In D. E. Blackman & H. Lejeune (Eds.), *Behaviour analysis in theory and practice: Contributions and controversies* (pp. 115–138). Hove, England: Erlbaum.
- Dymond, S., & Barnes, D. (1995). A transformation of self-discrimination response functions in accordance with the arbitrarily applicable relations of sameness, more than, and less than. *Journal of the Experimental Analysis of Behavior*, 64, 163–184.
- Fields, L. (1996). The evidence for naming as a cause or facilitator of equivalence class formation. *Journal of the Experimental Analysis of Behavior*, 65, 279–281.
- Galizio, M. (1996). Methodological issues in the study of naming. *Journal of the Experimental Analysis of Behavior*, 65, 286–288.
- Gathercole, V. C. (1979). The acquisition of more and less: A critical review. *Kansas Working Papers in Linguistics*, 4, 99–128.
- Gathercole, V. C. (1985). More and more and more about more. *Journal of Experimental Child Psychology*, 40, 73–104.
- Gopnik, A., & Meltzoff, A. (1992). Categorisation and naming: Basic level sorting in 18-month-olds and its relation to language. *Child Development*, 63, 1091–1103.
- Harnad, S. (1990). The symbol grounding problem. *Physica D*, 42, 335–346.
- Hayes, S. C. (1991). A relational control theory of stimulus equivalence. In L. J. Hayes & P. N. Chase (Eds.), *Dialogues on verbal behavior* (pp. 19–40). Reno, NV: Context Press.
- Hayes, S. C. (1994). Relational frame theory as a behavioral approach to verbal events. In S. C. Hayes, L. J. Hayes, M. Sato, & K. Ono (Eds.), *Behavior analysis of language and cognition* (pp. 9–30). Reno, NV: Context Press.
- Hayes, S. C. (1996). Developing a theory of derived stimulus relations. *Journal of the Experimental Analysis of Behavior*, 65, 309–311.

- Hayes, S. C., Gifford, E. V., & Wilson, K. G. (1996). Stimulus classes and stimulus relations: Arbitrary relational responding as an operant. In T. R. Zentall & P. M. Smeets (Eds.), *Advances in psychology: 117. Stimulus class formation in humans and animals* (pp. 279–299). Amsterdam: Elsevier.
- Hayes, S. C., & Hayes, L. J. (1989). The verbal action of the listener as a basis of rule-governance. In S. C. Hayes (Ed.), *Rule-governed behavior: Cognition, contingencies, and instructional control* (pp. 153–190). New York: Plenum.
- Hayes, S. C., & Hayes, L. J. (1992). Verbal relations and the evolution of behavior analysis. *American Psychologist*, 47, 1383–1395.
- Hayes, S. C., & Wilson, K. G. (1996). Criticisms of relational frame theory: Implications for a behavior analytic account of derived stimulus relations. *The Psychological Record*, 46, 221–236.
- Holland, P. C. (1981). Acquisition of representation-mediated conditioned food aversions. *Learning and Motivation*, 12, 1–18.
- Holland, P. C. (1990). Event representation in Pavlovian conditioning: Image and action. *Cognition*, 37, 105–131.
- Horne, P. J., & Lowe, C. F. (1993). Determinants of human performance on concurrent schedules. *Journal of the Experimental Analysis of Behavior*, 59, 29–60.
- Horne, P. J., & Lowe, C. F. (1996). On the origins of naming and other symbolic behavior. *Journal of the Experimental Analysis of Behavior*, 65, 185–241.
- House, B. J., Brown, A. L., & Scott, M. S. (1974). Children's discrimination learning based on identity or difference. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 9, pp. 1–45). New York: Academic Press.
- Hunter, G. (1974). Concepts and meaning. In W. B. Todd (Ed.), *Hume and the Enlightenment* (pp. 136–152). Edinburgh: Edinburgh University Press.
- Lowe, C. F. (1979). Determinants of human operant behavior. In M. D. Zeiler & P. Harzem (Eds.), *Advances in analysis of behavior: Vol. 1. Reinforcement and the organization of behavior* (pp. 159–192). New York: Wiley.
- Lowe, C. F. (1983). Radical behaviorism and human psychology. In G. C. L. Davey (Ed.), *Animal models of human behavior: Conceptual, evolutionary, and neurobiological perspectives* (pp. 71–93). New York: Wiley.
- Lowe, C. F., Beasty, A., & Bentall, R. P. (1983). The role of verbal behavior in human learning: Infant performance on fixed-interval schedules. *Journal of the Experimental Analysis of Behavior*, 39, 157–164.
- Lowe, C. F., & Horne, P. J. (1985). On the generality of behavioural principles: Human choice and the matching law. In C. F. Lowe, M. Richelle, & D. E. Blackman (Eds.), *Behaviour analysis and contemporary psychology* (pp. 97–115). London: Erlbaum.
- Lowe, C. F., & Horne, P. J. (1996). Reflections on naming and other symbolic behavior. *Journal of the Experimental Analysis of Behavior*, 65, 315–353.
- Lowe, C. F., Horne, P. J., & Higson, P. J. (1987). The hiatus between theory and practice in clinical psychology. In H. J. Eysenk & I. Martin (Eds.), *Theoretical foundations of behavior therapy* (pp. 153–165). New York: Plenum.
- Lowenkron, B. (1996). Joint control and word-object bidirectionality. *Journal of the Experimental Analysis of Behavior*, 65, 252–255.
- Lowenkron, B., & Colvin, V. (1992). Joint control and generalized nonidentity matching: Saying when something is not. *The Analysis of Verbal Behavior*, 19, 1–10.
- Mead, G. H. (1934). *Mind, self and society*. Chicago: University of Chicago Press.
- Michael, J. (1996). Separate repertoires or naming? *Journal of the Experimental Analysis of Behavior*, 65, 296–298.
- Morgan, C. L. (1894). *Introduction to comparative psychology*. London: Scott.
- Nelson, K. E., & Bonvillian, J. D. (1973). Concepts and words in the 2-year-old: Acquiring of concept names under controlled conditions. *Cognition*, 2, 435–450.
- Oden, D. L., Thompson, R. K. R., & Premack, D. (1990). Infant chimpanzees spontaneously perceive both concrete and abstract same/different relations. *Child Development*, 61, 621–631.
- Palermo, D. S. (1973). More about less: A study of language comprehension. *Journal of Verbal Learning and Verbal Behavior*, 12, 211–221.
- Palermo, D. S. (1974). Still more about the comprehension of "less." *Developmental Psychology*, 10, 827–829.
- Pilgrim, C. (1996). Can the naming hypothesis be falsified? *Journal of the Experimental Analysis of Behavior*, 65, 284–285.
- Pilgrim, C., Chambers, L., & Galizio, M. (1995). Reversal of baseline relations and stimulus equivalence: II. Children. *Journal of the Experimental Analysis of Behavior*, 63, 239–254.
- Pilgrim, C., & Galizio, M. (1995). Reversal of baseline relations and stimulus equivalence: I. Adults. *Journal of the Experimental Analysis of Behavior*, 63, 225–238.
- Pilgrim, C., & Galizio, M. (1996). Stimulus equivalence: A class of correlations or a correlation of classes. In T. R. Zentall & P. M. Smeets (Eds.), *Advances in psychology: 117. Stimulus class formation in humans and animals* (pp. 173–195). Amsterdam: Elsevier.
- Putnam, H. (1981). *Reason, truth and history*. Cambridge: Cambridge University Press.
- Putnam, H. (1992). *Renewing philosophy*. Cambridge, MA: Harvard University Press.
- Remington, B. (1996). The evolution of naming—just so! *Journal of the Experimental Analysis of Behavior*, 65, 243–244.
- Roche, B., & Barnes, D. (1996). Arbitrarily applicable relational responding and sexual categorization: A critical test of the derived difference relation. *The Psychological Record*, 46, 451–475.
- Ryle, G. (1949a). *The concept of mind*. London: Hutchinson's University Library.
- Ryle, G. (1949b). Meaning and necessity. *Philosophy*, 24, 69–76.
- Saunders, K. J., & Spradlin, J. E. (1996). Naming and equivalence relations. *Journal of the Experimental Analysis of Behavior*, 65, 304–308.
- Saunders, R. R. (1996). From review to commentary on Roche and Barnes: Toward a better understanding of equivalence in the context of relational frame theory. *The Psychological Record*, 46, 477–487.
- Saunders, R. R., & Green, G. (1992). The nonequivalence of behavioral and mathematical equivalence. *Journal of the Experimental Analysis of Behavior*, 57, 227–241.
- Saunders, R. R., & Green, G. (1996). Naming is not (necessary for) stimulus equivalence. *Journal of the Experimental Analysis of Behavior*, 65, 312–314.
- Schusterman, R. K., & Kastak, D. (1993). A California

- sea lion (*Zalophus californianus*) is capable of forming equivalence relations. *The Psychological Record*, 43, 823–839.
- Scruton, R. (1994). *Modern philosophy: An introduction and survey*. London: Mandarin.
- Searle, J. M. (1980). Minds, brains and programs. *Behavioral and Brain Sciences*, 3, 417–457.
- Sidman, M. (1994). *Equivalence relations and behavior: A research story*. Boston: Authors Cooperative.
- Siemann, M., Delius, J. D., Dombrowski, D., & Daniel, S. (1996). Value transfer in discriminative conditioning with pigeons. *The Psychological Record*, 46, 707–728.
- Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review*, 57, 193–216.
- Skinner, B. F. (1957). *Verbal behavior*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts.
- Spencer, T. J., & Chase, P. N. (1996). Speed analyses of stimulus equivalence. *Journal of the Experimental Analysis of Behavior*, 65, 643–659.
- Steele, D., & Hayes, S. C. (1991). Stimulus equivalence and arbitrarily applicable relational responding. *Journal of the Experimental Analysis of Behavior*, 56, 519–555.
- Stromer, R., & Mackay, H. A. (1996). Naming and the formation of stimulus classes. In T. R. Zentall & P. M. Smeets (Eds.), *Advances in psychology: 117. Stimulus class formation in humans and animals* (pp. 221–252). Amsterdam: Elsevier.
- Stromer, R., Mackay, H. A., & Remington, B. (1996). Naming, the formation of stimulus classes, and applied behavior analysis. *Journal of Applied Behavior Analysis*, 27, 409–431.
- Thompson, R. K. R. (1995). Natural and relational concepts in animals. In H. L. Roitblat & J.-A. Meyer (Eds.), *Comparative approaches in cognitive sciences* (pp. 175–224). Cambridge, MA: MIT Press.
- Vygotsky, L. S. (1978). *Mind in society: The development of higher psychological processes*. Cambridge, MA: Harvard University Press.
- Weiner, S. L. (1974). On the development of more and less. *Journal of Experimental Child Psychology*, 17, 271–287.
- Whitehurst, G. J., & Fischel, J. E. (1994). Early developmental language delay: What, if anything, should the clinician do about it? *Journal of Child Psychology and Psychiatry*, 36, 613–648.
- Wulfert, E., & Hayes, S. C. (1988). Transfer of a conditional ordering response through conditional equivalence classes. *Journal of the Experimental Analysis of Behavior*, 50, 125–141.
- Wynne, C. D. L. (1995). Reinforcement accounts for transitive inference performance. *Animal Learning & Behavior*, 23, 207–217.
- Wynne, C. D. L. (1997). Pigeon transitive inference: Tests of simple accounts of a complex performance. *Behavioural Processes*, 39, 95–112.